

THE ESP CONTROVERSY

By DOROTHY H. POPE and J. G. PRATT

WE HAD OCCASION to point out, in a former article (12) that a considerable portion of the material contained in this JOURNAL has been devoted to discussion, as distinguished from reports, of the research in extra-sensory perception and that some of these were in a critical vein. When the entire literature on the subject of parapsychology is taken into account, the articles dealing critically with the ESP work actually outnumber those which report original research. Since such a large part of the collective effort has been expended in this manner, the importance of judging the effect which the criticism has had upon the progress of the investigation is obvious.

There are two reasons why we wish to discuss this now: One is the fact that the literature of the controversy has by this time become sufficiently extensive that we should be able to generalize about it with considerable profit and so to derive some insight as to the present standing of the research and some guidance as to its future conduct. The second reason is that, writing as we are when the controversy has for the most part subsided, we have the advantage of being able to treat the critical phase of ESP literature in its entirety.

In view of the revolutionary character of the hypothesis of ESP for general psychology, it is not surprising that the literature of the controversy has, in this instance, been extensive. Indeed, it would have been surprising, after the experiments had received wide public attention, if considerable discussion had not developed. The history of science shows clearly that the claim for any unusual discovery will meet with objections to the degree that the principle discovered is unusual and the extent to which it receives general notice. Moreover, the ESP investigators, from the beginning, asked for criticism of their work, feeling that where the implications of the findings

would be so tremendous, every reasonable objection to the research should be examined and weighed.

SCOPE AND QUALITY OF THE CRITICISM

There has, of course, been adverse criticism of extra-sensory perception ever since the beginning of research in this field more than sixty years ago. But the scope of our survey will be limited to the particularly active period of eight years which began in 1934 with the publication of the Duke experiments. The critical articles, reviews, and letters to editors which appeared in various newspapers, journals, books, and magazines during this time totaled approximately fifty-nine, and the frequency of their appearance, it will be noted, is roughly proportionate to the amount of public interest in the research. In 1935 and 1936, this interest was negligible and the articles in the press numbered only four and one, respectively, for those years. The boom in criticism came in 1937, along with increasing publicity. In the late fall of '36, Professor Ernest Hunter Wright of Columbia had written a popular review of the ESP experiments for *Harpers* (20), a condensation of which appeared in *Readers Digest* early in 1937. The Book-of-the-Month Club extended popular interest by its choice of Dr. J. B. Rhine's *New Frontiers of the Mind* (13) as their October, 1937, selection. The radio, capitalizing on current interest in ESP, carried programs over nation-wide hookups in 1937 and 1938, and most popular magazines and newspapers ran articles on the subject. Concurrently with all this publicity, the number of critical articles in the ESP literature increased to thirteen in 1937 and twenty-nine in 1938, nearly all of them representing new names whose owners were vigorous in their attack upon the research.

This same upsurge of interest in ESP was apparent at psychological meetings where the discussion of ESP had at first been heard only in the hallways and offstage, as it were. In 1937, the topic began to make its way into the programs, and in the spring of '38, several sectional meetings of the American Psychological Association listed papers on ESP, critical or otherwise. Finally, in the fall of that year, at Ohio State University, a special symposium on methods in ESP tests was arranged under the Association auspices. This event represented a climax in the controversy; and after it,

critical interest declined, not only at the psychological meetings, but in the mind of the public as well, a decline which was evidenced by a diminution in the number of critical articles to eight in 1939, four in 1940, and none at all, to our knowledge, since that time.

Mere amount of criticism, however, in terms of numbers of articles or pages affords no adequate conception of what the student of ESP wishes to know about the controversy. It is more important, among other things, to note who the principal critics were and what was the nature and quality of their contribution. The critics were, almost without exception, members of that broad classification known as "the psychological profession." This fact, of course, is quite as it should be, since the ultimate goal of parapsychology is the coordination of its findings with those of general psychology. That the controversy was, at this period in the history of the research, primarily the concern of psychologists may, therefore, be taken as an indication of real progress. In the period prior to 1934, never had half so much attention been accorded to the ESP research—not even to purely destructive criticism of it. But the preponderance of psychologists among the new critics indicated that the profession had at least been put on the defensive and a clear-cut issue had been drawn for the student of psychology.

The tone of the criticism in 1935 and 1936, which may be called quiet years, was moderately well tempered. But as public interest arose in '36, '37, and '38, there was noticeable an increasing irritation conveyed by explicit statement of condemnation, as exemplified, for instance, by Dr. Kellogg's designation of ESP as a "craze" (9). This emotional turn seems best interpreted as the professional psychologist's reaction to the frequent bombardment of questions regarding ESP which students and laymen leveled at him during the period when ESP was so frequently mentioned in the press and on the air. Given a natural doubt as a result of his training, the psychologist, unfamiliar with the experimental work as he doubtlessly was in most cases, must unavoidably have reacted with annoyance. When he set forth to clear the air of these disturbing claims, it was hardly to be expected that he could restrain his personal feelings without some difficulty.

Moreover, the claims of ESP genuinely conflict with the intellectual heritage of most psychologists. Since the 17th century,

if not indeed since ancient Greece, the social sciences have been predominantly influenced by the development of the more successful physical sciences. The result has been an increasing emphasis upon the more objectively checked processes of personal activity, the sensory and motor functions. Mental life beyond the senses is less subject to measurement in ultimately physical terms. In fact, the very erratic, spontaneous nature of extra-sensory perception sets it off as almost contrary to physical occurrences. Good systematic classification required that it be ruled out. It was easier to make the senses the only channels to cognition of the external world. This point of view has been symbolized by the classical assertion that there is "nothing in the intellect which was not first in the senses."

The facts of ESP therefore come into direct conflict with the prevailing scientific assumptions of centuries and this incompatibility in mode of thought, rather than any inadequacy of evidence, has made acceptance of ESP especially difficult for the psychologist. Ours is an age which has learned to see the world one way, and according to that one way, there can be no such thing as perception without the senses.

REVIEW OF THE CRITICISM

The specific criticisms of the work are, of course, the more important consideration in this review. They fall most naturally into three general groups: (1) those concerned with the means of evaluating the ESP results with respect to chance—the statistics and mathematics used; (2) a general group dealing with the adequacy of the experimental procedures; and (3) those which may be characterized as pertaining to the logic of the interpretation of the ESP hypothesis, especially as it is related to the success or failure of different experimenters. These three topics are mentioned in an order which is itself of significance since it represents the general chronology of the criticism. Up through 1937, the objections to the research were chiefly concerned with the mathematics; through part of 1937 and in 1938, they were mainly directed at the experimental methods; and in the latter part of '38 and on into '39, they were concerned more especially with the interpretation of results. Each of these trends ran its course and each, as may be seen from what follows, reached a somewhat natural climax after which the issue apparently ceased to exist.

The mathematical phase of the criticism is represented, strangely enough, not by mathematicians but by psychologists. Not a single mathematician, in this period, raised his voice against the techniques of evaluation, and those criticisms coming from psychologists were sometimes obviously based on such uncertain ground as to require, in several instances, their subsequent withdrawal. The first critic of the mathematics was Dr. R. R. Willoughby, then of Clark University, who presented three papers on ESP in 1935 (17, 18, 19). His main point was that the method of evaluation in use was an improper one and he proposed an alternative. Later, however, he reversed his position on this point and still later, in 1940, conceded that the statistical issue was then irrelevant (15: ch. 8).

Dr. C. E. Kellogg, of McGill University, was another who, in his first article (8), suggested an alternative method of finding the probable error and withdrew it in a later discussion (10). His contention—that the standard deviation of the observed data should be used rather than that based on the theoretical binomial distribution—is not insisted upon in his last paper stating his critical position (15: ch. 8) and it seems safe to infer that he has given up that point also. At any rate, he recognizes clearly that portions of the evidence differ significantly from chance. Obviously, from the standpoint of whether or not ESP occurs, any further discussion would be merely academic.

In any case, a satisfactory answer was provided, for the question which Kellogg raised, by Greenwood's empirical chance study of card matchings (5). This study consisted of 500,000 matchings of ESP cards against actual calls made by subjects in earlier tests with which they were not intended to be paired. He found the frequency of correct correspondences to be very close to binomial expectation; that is, the calls represented a chance series. On the basis of this study, Greenwood was able to show that the methods actually in use in the ESP research—methods based on the binomial expectation—were more nearly applicable than those proposed by Kellogg.

The question arose, too, regarding the accuracy of the probability of one fifth attributed to each trial in the ESP tests. In other words, it was doubted whether five hits per twenty-five was the correct expectation on the theory of chance. Mathematicians and ESP research workers, however, were able to show by empirical control

series and by actual mathematical proof that this objection to the mathematics was completely groundless.

These by no means exhaust the range of the mathematical questions and criticisms which were raised; but there was only one other which was sufficiently relevant to deserve mention. This one—a suggested correction in the method of computing the standard deviation—came principally as a result of Kellogg's criticism and led to the use of an alternative technique by ESP workers for a time. Later on, the large empirical chance series by Greenwood, which was mentioned above, settled the issue in favor of the method first in use, and this correction of two percent of the standard deviation was discontinued.

We have remarked that those who criticized the statistics of ESP were not mathematicians, and during the mathematical phase of the period of controversy, this was literally true. Recently, however, there has been an exception, one that is entirely "out of line" with the other members of his profession who have generally approved the methods in use by ESP workers. It was some three years after the mathematical issue had been laid to rest by the mathematicians themselves that Dr. Willy K. Feller (4) attempted to dispose of the case for ESP on the ground that the cards were inadequately shuffled, the data improperly selected, and the experiments spuriously significant because they were terminated at favorable stopping points. However, Greenwood and Stuart (6) were, without difficulty, able to show from the research reports already published that these criticisms could not account for the ESP results nor invalidate the conclusions. Feller has not resumed the discussion since that time.

In assessing the value of the mathematical controversy, it is fair to say that while mathematicians did not openly criticize the ESP work, they did, in response to requests for assistance and advice by ESP workers, contribute greatly to the integrity of the work by offering suggestions for proper procedures and giving their assistance when mathematical difficulties arose. In fact, the ESP controversy brought forth a quantity of original statistical research. When the subject of ESP statistics finally was raised in 1937 at the meeting of the Institute of Mathematical Statistics, a now familiar press release was issued by the president of that body approving, in gen-

eral, the validity of the statistical analyses of the ESP work and ending with the statement: "If the Rhine investigation is to be fairly attacked, it must be on other than mathematical grounds" (1). This release and the publication of several other explanatory articles marked the abrupt cessation of mathematical controversy in relation to ESP. Appropriately, Professor E. V. Huntington, in the most outstanding of these articles (7), inquired: "If mathematics has successfully disposed of the hypothesis of chance, what has psychology to say about the hypothesis of ESP?"

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The critics had already begun to scrutinize the experimental adequacy of the ESP methods even before the authoritative ruling of the Institute of Mathematical Statistics was made public, and in 1938, the year following that statement, there was a blast of criticism on this new note. Most of it had to do with a total misunderstanding by the critics of the use of the commercial ESP cards in the techniques employed in the research. The production of the ESP cards had, in a general way, been supervised from the Duke Laboratory; but in spite of the warnings and instructions given, the manufacturer was not able to avoid a certain amount of warping of the cards which, while it did not show up in proof, appeared some time after manufacture. While this warping of the cards was not at all obvious to the average person, the symbols could be read from the backs of the cards if they were held at a certain angle to the source of light.

The decision had to be made by the members of the laboratory staff as to whether the entire stock should be discarded or whether it should be released, with appropriate instructions, of course, concerning the use of the cards for experimental purposes. It was decided that the latter course was advisable, if not unavoidable, since a reprinting could not be obtained. For the layman's purposes the cards were satisfactory for personal testing and illustrative material; only the person who was seeking to deceive was likely to avail himself of the possible cues. And for scientific testing, no cards could be considered "perfect" and none could be reliably used without screening, anyway. An announcement in the next number of the JOURNAL OF PARAPSYCHOLOGY suggesting the use of opaque screens with the ESP cards afforded sufficient warning. For that matter, experienced investigators had long since ceased to trust to

any cards, however free from defects they might seem to be at the beginning of a test, and no research had been reported from the laboratory in which the primary conclusions had been based on tests made with unscreened cards.

But in spite of the notice in the JOURNAL and with utter disregard for the emphasis upon screening of cards in the ESP reports, Dr. S. H. Britt read a paper at the spring meeting of the eastern branch of the American Psychological Association in which he made a sensational demonstration of how the commercial ESP cards could be "read" from the backs. Other critics—Kennedy, Skinner, Wolfe, Gulliksen—joined in, all completely ignoring the fact that the cards, however faulty, could not give sensory cues *if they were concealed behind opaque screens*. This was not the only evidence abroad that the actual reports had not been carefully read.

So much was made of these defective cards and so much attention given to ESP research in 1938 that the A. P. A. arranged for a symposium on experimental methods of testing ESP (3). It is perhaps indicative of the temper of the times that Kennedy, then one of the most active critics of the ESP work, was appointed the chairman. His own paper—suggesting that recording errors would account for the ESP results—was so completely and adequately answered by Dr. Gardner Murphy, who followed him, that this brand of criticism has not been raised since that distinctive occasion. Moreover, Gulliksen, who led the attack on the issue of experimental inadequacy, which had been the main current charge against ESP for a year, agreed on the floor that the experimental methods as described by Rhine were, indeed, acceptable to him.

Thus ended the second phase of the criticism, the problem of experimental methods. There was little or no questioning, by this time, of the mathematics in use and the A. P. A. symposium more or less closed the issues in existence up to that time. It was manifestly a turning point in the ESP controversy and few critical papers have been written since then. It had the effect of clarifying the issues and it accomplished a great deal toward the general understanding of the character of the ESP research.

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The third period of criticism overlaps the second as the second overlapped the first. It was in the summer of 1938 that Dr.

Clarence Leuba (11) voiced his doubts as to the conclusiveness of the ESP results on the grounds, first, that an experimenter could take advantage of a convenient run of luck by "optional stopping"; i.e., by stopping his experiment when results had been favorable; and second, that due consideration was not given to the unsuccessful tests by experimenters who had tried to get positive results and had failed.

The optional stopping topic was current in the discussions of ESP throughout 1939 and was one of the main issues in the ESP symposium held by the Southern Society for Philosophy and Psychology that year. Indeed, it is one of the few published criticisms which actually led to an alteration of methodology, even though, it should be said, such alteration was not essential for the establishment of the ESP hypothesis. Briefly, the question of optional stopping was solved, so far as the present period of controversy is concerned, not so much by argument as by methodological adjustments. Experiments were simply given, in advance, an estimated range which did not allow for the option in question. However, the previous policy of *publishing all the investigations made to date* under the specified conditions was itself, for the time, an effective answer to the question raised, even though technical improvement was possible. Since a complete report cannot be criticized for selection, this question, too, was only an academic one and did not concern the main issue of the ESP hypothesis. All that remained for the critics to say was what Kellogg finally did say, in effect, in his 1940 critique (15: ch. 8): "Keep on going under present test conditions and you will probably find that you simply will not get any more evidence of ESP." Every issue of the JOURNAL OF PARAPSYCHOLOGY since that time has been testimony to the failure of his prediction.

Of far more concern among psychologists and, through them, among critical lay-readers, was the other charge, that not enough importance was attached to the "negative series" the results of which did not show ESP. By some odd type of reasoning, certainly not mathematical, it seems to have been felt that a failure on the part of one psychologist to obtain evidence of ESP when he administered his tests—whatever may have been the conditions or methods he used—canceled out the favorable findings of another. In this regard, people seem to have been misled into comparing ESP

with the physical sciences in which, if one investigator does not confirm another's work, there is something obviously wrong somewhere. It is forgotten that human beings are not like inorganic bodies and substances; they are exceedingly variable and subject to the most subtle influences. The conditions of the experiment are very important, and individual differences sometimes extreme. Consequently, one psychologist's failure may actually mean nothing regarding another's success.

The most vigorous critic in this aspect of the research was an Englishman—not a psychologist, but a college teacher of mathematics—Mr. S. G. Soal, who had himself conducted a long series of ESP tests from which he had not obtained a significant positive deviation. Contrasting his results with those of American investigators, he threw out strong implications against the propriety of reaching favorable conclusions from work that did not agree with his own. Since 1940, however, this critic has been led to reverse his stand (16), and that, strangely enough, by his own discoveries which are in themselves among the most interesting of recent years. At the suggestion of Whately Carington, he looked for displacement effects (that is, ESP hits on neighboring targets rather than on the intended targets) and he has turned up highly significant evidence of ESP. This evidence has apparently continued to accumulate even under the difficult conditions induced by the war. Hence, the most conspicuous “failure” has turned into what is perhaps at present the most remarkable “success.” Little is heard or probably will be heard of this third stock argument against ESP research, for this one instance goes far to remind the critics in general that determination of success and failure may lie even within the experimenter himself, let alone those other variables—subjects, conditions, and methods of analysis. “Failure,” then, may be an indiscriminate term.

From a consideration of these three phases of the controversial era of ESP, it would appear that events, more than arguments, turned the tide of criticism—events which have brought into focus, or at least brought to the attention of the audience concerned, the arguments or facts which would have otherwise been inadequate to stem the criticism. Specifically, it was the official pronouncement by the Institute of Mathematical Statistics, the A. P. A. symposium,

and Soal's discovery of displacement in his data that have marked the turning points, if not, indeed, constituted them.

This is not to say that there is general acknowledgment of the finality of the findings or agreement as to the interpretation of the results. It is safe to say, however, that there is a fairly widespread recognition of the scientific character of the research and an increased respect for the way it is being done. When, in the fall of 1939, the seven leading psychological critics—Willoughby, Kellogg, Wolfle, Gulliksen, Kennedy, Lemmon, and Thouless—were invited to produce their criticisms for incorporation into the book, *Extra-Sensory Perception After Sixty Years* (15), a marked change in attitude from that of 1937-38 appeared, one that was obviously more thoughtful and restrained than that which some of the same writers had shown in their earlier critiques. In fact, it should be said (particularly because objective criteria on such matters are so few) that only three of the seven accepted the invitation. This, it must be remembered, was only a year or two after the time when those who now declined had been very actively engaged in vigorous criticism. Of the three who accepted the invitation, Dr. R. H. Thouless, of Cambridge University, never a completely destructive critic in the first place, sounded a wholly constructive and favorable note. Dr. V. W. Lemmon, of Washington University, also customarily temperate in his criticism, was restrained and relatively neutral. Only Dr. Kellogg continued to reject the ESP research without compromise; but the force of his criticism had been greatly moderated and was by no means difficult to cope with. In a word, the response of the seven critics tells more than any other single event of the end to which ESP criticism has finally come: mainly silence, with some shift toward favorable, or at least neutral, attitudes and an element of restrained but persistent die-hard rejection.

EFFECT OF THE CRITICISM ON THE RESEARCH

Turning from these more specific features of the ESP controversy to the matter of its general effects and significance, we find ourselves on more difficult ground. Undoubtedly the past eight years have witnessed marked advances in the methodology of parapsychological research, as regards both the statistical evaluation of the results and the experimental methods. The question which we wish finally to

raise is this: To what extent have these advances come about as a result of the published criticism?

An answer to this question cannot be given without our first calling attention to the fact that the best type of criticism of the research has generally not been published; consequently, it never became a part of the public discussion, that exchange of articles which have been grouped under the general term, the "ESP controversy." In 1938, when the discussion of the ESP results was at its height, the editors of the JOURNAL OF PARAPSYCHOLOGY (2) pointed out that the best criticisms of the experiments had been offered by fellow investigators. Next, as a class, in fruitfulness of their suggestions came the active students of the research and the intimate colleagues of the investigators who offered their constructive criticisms of the work as it progressed. Much of the discussion within and between these two groups took place either in conversation or through private correspondence so that there was nothing for the scientific world to see except the *effects* produced on the research itself. The general result was such real progress toward complete safeguarding of the tests that the first publication from Duke University contained some of the soundest evidence of ESP which has yet been produced. Many of the hostile critics who later published attacks upon the experiments raised only such objections as could be effectively answered by the evidence already at hand. It was the rule rather than the exception that the objections of the critics were already being dealt with even before they appeared.

It seems to us that this editorial commentary is still a good evaluation of the role of the critics, and inasmuch as we have dealt here exclusively with the adverse attacks upon the research which were featured in the actual controversy and which had been anticipated in the natural progress of the experiments, there is not a great deal left to credit to this class of critic. Moreover, those objections which had some basis in fact were presented in a manner calculated to obstruct and weaken the research, and the resolution of the problem usually fell upon someone more constructively disposed toward the field of parapsychology. But the issues were eventually settled; and, in fairness, the critics may properly be credited with having helped to produce certain refinements of methodology which the investigators themselves had either overlooked

or had not regarded as essential. The provisions against optional stopping and for independent recording are in this classification. They are precautions which are now taken in most ESP research but not expected of other fields of experimentation. Whether they are requisite for all ESP work is still perhaps a debatable issue.

To close the account, it is equally necessary to total up the losses which parapsychology has encountered through the ESP controversy. The first item is that of the time and effort which have gone into answering critics and the attempt to anticipate what they might later have to say. That this time and effort have been onerous is self-evident from the amount of the ESP literature which has dealt with or touched upon the controversy. It is impossible to say exactly what difference it would have made for the history of parapsychology if the investigators had been able to use the many hours so spent in further research. We can only say with certainty that the controversy has been a great drain upon the time and energies of the experimenters.

It has also caused the experimenters to change their general approach to ESP testing. The change has most noticeably affected the psychological atmosphere of the tests and the experimental freedom used by the investigators. The term "psychological atmosphere" is used to refer to those delicate social factors which are difficult to describe objectively but which research workers have long recognized as essential to the successful demonstration of ESP. They inhere in the condition of rapport between the investigator and his subject. A spontaneity of interest in the test and its outcome has been the essential condition most commonly specified. There can be no doubt that as investigators have more and more planned their tests in the light of what the critics would think, they have so formalized the procedures that it has been increasingly difficult, if not impossible, to arouse a favorable type of interest in the tests.

This sacrifice of experimental freedom to the critics was made in an effort to remove even the appearance of evil from the research. Because there have been objections to tests which were made with the cards in sight of the subjects, the ESP researchers have gone to the other extreme of *never* doing tests in this fashion. Because the use of exploratory tests led to the charge of keeping the good results and throwing away the bad ones, preliminary trials for the purpose

of selecting suitable subjects and for "warming up" have been abandoned almost altogether. Because unconsciously motivated errors were attributed to the investigators, experiments were planned with a careful division of labor between two observers so that errors short of collusion were impossible. By such changes as these, investigators made concessions which silenced the critics but which, at the same time, may have served to hamstringing their own activities since the occurrence of ESP is subject to certain influences which are known to be easily affected by the experimental conditions.

In any event, the first step is to secure the effect to be studied. It is like the old recipe for cooking 'possum: first you catch the 'possum. Accordingly, it may well be that in ESP testing the informality and spontaneity which accompany unstudied preliminary conditions is the surer way of leading up to an adequately safeguarded, crucial demonstration of the phenomenon. To many of us, the Duke experiments appear to suggest that this is the case, for it was out of such informal beginnings as those with Pearce, to cite a familiar example, that the Pearce-Pratt series (14) and other rigorously safe-guarded and successful projects emerged.

Another large item on the debit side is the effect which the controversy has had upon professional groups outside the field of psychology and upon the general public. Individual psychologists are, after all, in a fair position to form their own opinions about a body of evidence which is so vitally related to their branch of science. Other professional groups would be much more likely, however, to be influenced against the conclusion that ESP occurs simply because there are professional psychologists who, at least at one time, emphatically asserted that the evidence was unsound. The non-psychologist may not be aware of the relatively unsettled state of the whole science of psychology. When he sees or hears a man who has been trained in that branch of study dispose of the entire case for ESP, he is likely to agree out of deference for the critic's position. There are known instances in which this has been so and we can only guess how many more people have been influenced to take their opinions of the case for ESP from unjustified attacks on the experiments by psychologists who have published their criticisms with an air of finality that has sometimes approached the ridiculous.

By way of final account, let it be said that the investigators will,

as ever, continue to welcome criticism of their results and will doubtless, in some instances, deserve it. But they may be excused for holding to the hope that future criticism will come closer to dealing with real issues in the research than it has in the past. The best prospect for the progress of the research would now seem to require that the main points of this past controversy—many of which need never have been raised—be regarded as settled for good. Thus will investigators and critics alike be free to use their time most constructively in regard to the real issues of the evidence and the great problems of its interpretation that lie ahead.

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